

of the Royal Physical Society. As an account of the observations and experiments, which were numerous and detailed, will appear in the next part of the Proc. Roy. Physical Soc., an indication of their bearing is all that is necessary for the present.

(1) As regards *Nosema apis*, the authors have been "unable to recognise any causal relation between the presence of this parasite and the disease." Healthy stocks with no signs of disease have been found to be heavily infected by the protozoon, and that over prolonged periods. Numerous stocks have exhibited unmistakable symptoms of Isle of Wight disease, and yet no trace of *Nosema* has been found in them. This was markedly the case in the Deeside outbreak. Lastly, deliberate infection of a stock with *Nosema* did not produce the recognised symptoms of the disease. "*Nosema* may be a contributing weakening factor, favouring in certain cases the development of this disease, but we have not found that it is an essential factor."

(2) As regards the infectiousness of Isle of Wight disease: If it be allowed that *Nosema*, with its readily transported spores, is not the prime cause of the disease, the supporting evidence of infectivity is weakened, and the direct evidences must be examined more critically. The authors have watched in detail the natural course of Isle of Wight disease in three independent localities, and have followed the history of untainted swarms placed in contaminated hives and fed on contaminated honey. They have found no indubitable evidence of the infectiousness of the disease, although the indications seem to be that it is "probably infectious"; but in any case they are assured that it is "not necessarily conveyed by mere contact with contaminated hives or combs, or by feeding upon contaminated stores."

It is a point of some interest and importance that, on account of the unsatisfactory nature of experiments on a small scale in artificial conditions, the above results are based on observations and experiments upon hive bees living in natural conditions.

JAMES RITCHIE,

(Hon. Secretary, Royal Physical Society).

Edinburgh.

REGARDING Dr. J. Ritchie's communication, it would seem well to await the published paper of Messrs. Anderson and Rennie before making detailed remarks. Also, as Dr. Ritchie is not the direct author of the paper, it is inadvisable to bring in a third party. However, it is most surprising, to say the least, to learn that "Isle of Wight" bee disease is not considered to be infectious. How, then, has the disease spread all over Great Britain and most of Ireland during the last ten years? The statement of the non-infectivity of the disease is emphatically inaccurate. Dr. Ritchie writes of the "unmistakable symptoms" of the disease. But, what are the characteristic symptoms? The investigators working under the Board of Agriculture, in their reports of 1912 and 1913, showed conclusively that there were no well-marked differential symptoms of "Isle of Wight" bee disease. This was also pointed out in my article in NATURE, and the reason for this is obvious, namely, the limited range of expression of the bee, as was also mentioned in my article. Of the workers contributing to the reports of the Board of Agriculture, two were bacteriologists, two were protozoologists, and one was an expert bee-keeper. Many field experiments as to the pathogenicity of *Nosema apis* were conducted, and the investigators were unanimously of the opinion that "Isle of Wight" bee disease is microsporidiosis. Apparently Dr. Ritchie and Messrs. Anderson and Rennie have quite overlooked the importance of parasite carriers, a subject

which was carefully pointed out in my article and in the Journal of the Board of Agriculture, Supplements Nos. 8 and 10. Healthy carriers of most parasitic diseases are known.

As to "drastic recommendations," the simple elements of sanitation only were suggested, about which there can be no dispute. The destruction of hives was not suggested in my article. Regarding the experiments of Mr. J. Anderson and Dr. J. Rennie, there is no statement in the above letter as to what stages of *Nosema apis* were used by them.

These remarks must suffice for the present. My article was written after ten years' personal investigation of "Isle of Wight" bee disease, in nearly every part of Great Britain. Judging from Dr. Ritchie's letter, the paper of Messrs. Anderson and Rennie appears to contain little but negation. F.

Preventive Eugenics.

THE writer of the valuable article in NATURE of April 6, on the report of the Royal Commission on Venereal Diseases, has given it the title of "Preventive Eugenics," a term for which I am responsible, defining it as "the protection of parenthood from the racial poisons," by which latter I mean all such agents as, injuring the individual, injure also the next generation through him, or her, as parent.

Syphilis is, of course, an example of a racial poison, and your writer's protest against the term "hereditary syphilis" is most welcome to one who has made such protests for many years. As Dr. J. W. Ballantyne has said, the term is "an insult to heredity." It indicates the persistent medical and popular blindness to the ante-natal stage of human life. All syphilis is acquired syphilis, an infection of which the date may be ante-natal, when we inexcusably call it "hereditary," or post-natal, when we call it acquired, the fact being too obvious for even the "idols of the forum" to obscure. The Commissioners should have condemned the false term, and used "ante-natal syphilis" instead.

The point is not only academic. Eugenists who have had no medical, much less obstetrical, experience, unaware of the fallacy involved, have assumed much infant mortality to be due to bad heredity, and thus to be an instance of natural selection, when, in fact, ante-natally acquired infection of syphilis was responsible. This grave error is involved in the biometrical publications on infant mortality throughout, and has long discouraged the efforts now being made, at last, to save the infants who are our national future.

C. W. SALEEBY.

Royal Institution, W., April 8.

Atmospheric Electricity.

IT would be interesting to know if any reader of NATURE has made observations similar to those made here on the afternoon of April 14.

A large thundercloud was just passing off in the east without having produced any obvious thunderstorm phenomena. The sky overhead was occupied by cirrus, while a second thundercloud was coming up in the west. It was found that sparks, one of them certainly reaching 2 or 3 mm. length, could be drawn from the metal of a Besson comb nephoscope, supported on a wooden stand, with the comb at a height of 3½ metres above an asphalt roof (itself 12 metres above ground), on which observer and nephoscope stood. The leaden roof of a wooden cistern casing yielded similar results, but the most surprising observation was that a Campbell-Stokes sunshine recorder, bolted and cemented to a concrete parapet extending about a metre above the asphalt, also gave

quite appreciable sparks during the period of activity. The charges took fifteen or twenty seconds to build up after discharge, and the experiment was repeated very frequently.

The second thundercloud produced two peals of thunder and a slight shower, soon after which the abnormal electrical conditions ceased to manifest themselves, about three-quarters of an hour after they were first noticed.

R. A. WATSON WATT.

Meteorological Office, South Farnborough,
Hants, April 15.

The Influence of Tides on Wells.

REFERRING to Mr. Jas. Kewley's letter in *NATURE* of April 13, it is not unusual for the water in wells to rise and fall with the tides when such wells are near the sea. But is it necessary to assume that the phenomenon is due to the weight of the incoming tide compressing the underlying strata, as suggested by Mr. Kewley? Surely it may be sufficiently explained on the assumption that as the rising tide is a rising head of water it, without necessarily compressing the rocks beneath, tends to compress the air and replace the less dense fresh water included in the interstices and fissures, thus affecting the water-level of any contiguous well. In this connection may I direct attention to a letter of mine on "Tidal Action of the Earth's Crust," published in the *English Mechanic*, June 11, 1909?

CECIL CARUS-WILSON.

PHYSIOLOGY IN THE WORKSHOP.

IN the never-ending struggle between capital and labour, or rather between employers and workmen, the points of dispute have been largely concerned with the hours of labour and wages, the employers trying to obtain as long hours for as low wages as possible, while labour has struck for a shortening of hours with increased wages. Labour is thus regarded as a commodity, to be bought as cheaply and sold as dearly as possible.

In most of these disputes it would seem that both sides have lost sight of the fundamental conditions of their own prosperity. It is, after all, of little account to the employer that he should be able to buy cheaply so many hours of other men's lives. The only factor which really concerns him is that he should be able to produce as large a quantity of his goods as possible at as small a price as possible, reckoning both rent of capital and cost of labour. An implicit assumption seems always to be made that the more hours of a man's life the employer can buy for a certain sum, the cheaper will be his cost of production. But labour also is concerned in the cost of output. It is a truism that when business is slack, *i.e.* when the profits are small, strikes are few and far between, the workers recognising that it would be better in many cases to close down works than to give them increased wages. Both employers and workmen are therefore concerned that the industry in which they are engaged should be as prosperous as possible, *i.e.* that production should be as cheap and rapid as possible. To this end both parties should co-operate. The only divergence of view which is reasonable should occur later when the question arises of the division of the profits, *i.e.* as to how much

should be assigned to labour and how much for management and rent of capital.

Both sides are therefore interested in the efficiency of labour and its use to the best possible advantage—the employer in order that he may obtain as great a production at as small a price as possible; the workman that he may be able to earn enough to keep himself in comfort, while allowing some time in the day or week for recreation and the enjoyment of life.

It is remarkable how little attention has been paid in this country to the problem of how to use labour to the best possible advantage. The appearance of a Memorandum on "Industrial Fatigue and its Causes" (Cd. 8213, Wyman and Sons, Ltd., price 1½d.), which has been drawn up and issued by the Health of Munition Workers Committee, is therefore of extreme importance at the present time, since its object is to point out the only method by which increased efficiency of production can be attained.

In this pamphlet it is shown that the problem of scientific industrial management is fundamentally a problem in industrial fatigue. For the continued efficiency of an animal or man, rest must alternate with work, and the periods of rest and work must vary with the type of work involved. This elementary principle is acted upon generally in our management of horses. The report is a plea for its application also to the case of man. We cannot get the utmost possible work out of man or horse unless this principle is taken into account. We have thus to determine in the case of man what are the maximal efficiency rhythms for various types of work and workers. For work in which severe muscular effort is required it seems probable that the maximal output over a day's work and the best conditions for the workers' comfort and maintained health will be secured by giving short spells of strenuous activity broken by longer spells of rest, the relative amount of time devoted to resting being greater than in employments in which nervous activity is more prominent or more complicated.

The truth of this statement is well illustrated by an anecdote recorded in the Memorandum before us. Two officers at the front competed in making equal lengths of a certain trench each with an equal squad of men. One allowed his men to work as they pleased but as hard as possible. The other divided his men into three sets to work in rotation, each set digging their hardest for five minutes only, and then resting for ten until their turn came to dig again. The latter team won easily. Another instance is that of a munitions factory, where men engaged in the severe work of moulding were required to rest fifteen minutes in every hour of work. The men objected to this long spell of rest in each hour because the work was piecework, so that the manager had to make the hourly rest compulsory and appoint a foreman to see that the regulation was complied with. As a result of this the output per hour was found to be actually increased.

It is evident that the optimum working rhythm for each kind of work can only be determined by